

GEOPHYSICAL VARIABLES AND BEHAVIOR: XXVI.  
A RESPONSE TO RUTKOWSKI'S CRITIQUE OF THE  
TECTONIC STRAIN HYPOTHESIS FOR UFO PHENOMENA

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*Summary.*—The tectonic strain hypothesis for many reports of UFOs (UFORs), primarily odd luminosities and metallic-looking phenomena, has been criticized on the basis of inadequate data. This reply begins with the distinction between the empirical basis for the association between UFORs and seismic activity, the hypothesis, and laboratory experiments. It is emphasized that criticisms of data should be based upon empirical criteria rather than value judgments about scientific credibility. Multivariate and bivariate analyses have indicated systematic relationships between UFORs and earthquake measures within several different areas and for different historical periods. However, the physical mechanisms for the generation of individual UFO events and their relationship to UFORs require closer examination.

Rutkowski (1984) has criticized the tectonic strain hypothesis for UFORs (reports of unidentified flying objects) on the basis of its apparent lack of intuitive appeal, questionable data base and the requirement for multivariate statistical techniques. Since his approach reflects some attitudes rather than a methodology, a rebutal is required. If Rutkowski's criteria were applied to any other new area of research, progress would be stifled from the onset. Since the basis for the TST (Persinger, 1976, 1979b; Persinger & Lafreniere, 1977) is well documented by empirical analyses (Persinger, 1980a, 1983d, 1983f, 1983g; Persinger & Derr, 1984), his comments can be instructive.

Recognition of the rationale for the theory is primary. UFORs are considered to be a complex problem that involves several different disciplines. Such problems require a careful distinction between the conceptual understanding of the problem-solver and the technical expertise of the specialist. My approach is from the perspective of the problem-solver. There are two sets of data, UFORs and seismic events. They vary together in time. If the two are systematically related then specific changes within one set of events should be reflected in the other. At present, the hypothesis about tectonic strain is a conceptual metaphor through which we might visualize the causal process. Ultimately the hypothesis must be tested by geophysicists through their instrumentation and within their language system.

Unfortunately, Rutkowski's critique begins with a gross misrepresentation of the hypothesis. Contrary to his statements, the hypothesis states that UFORs

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are generated by strain fields; they are induced by changing stresses within the earth's crust. Earthquakes are hypothesized to be consequences of these fields as they move through susceptible regions. Conspicuous seismic activity can occur at great distances from the area within which a strain field has previously generated UFORs. Rutkowski's interpretation that "the theory incorporates luminous effects that are a great distance from the source" is not correct. The distal earthquake is not the source, rather both the UFORs and the earthquakes themselves are related to a third factor: the movement of strain fields (Persinger, 1983f).

Although the existence of complex stresses within the earth's crust and their consequences within rock structures (strain) are fundamental concepts in seismology (Bullen, 1965; Rikitake, 1976), the direct measurement of these processes is more difficult (Zoback & Zoback, 1981). Usually an increase in regional tectonic stress and strain is inferred by the later occurrence of earthquakes or their continuation (aftershocks) following particularly large quakes. Strain fields that change rapidly (hours to days), due to either a moving or time-varying process, are more difficult to measure. Of course, there are clear exceptions to this statement such as the pervasive and periodic strain fields produced by (lunar) tidal factors (Roosen, Harrington, Giles, & Browning, 1976). Optimally, the concept of a strain field should be coupled with empirical events, such as the seismic events of very small magnitude, now below routine detection, that might occur at the time and within the same locus as the UFOR.

The second error in Rutkowski's conceptual framework is his failure to differentiate the hypothesis (the strain field) from the empirical analyses. The hypothesis was derived from the conspicuous relationship in time between UFORs and earthquake activity within a region; the relationship is evident for different regions and for different historical periods (Persinger, 1983c, 1983d, 1983e, 1983f, 1984b). Entry of geophysical variables, such as measures of geomagnetic fluctuations (Persinger, 1981a, 1983d) are empirical results from carefully executed multivariate analyses. An approach that would indict the inclusion of variables simply because they "have not been demonstrated to be related to other geophysical phenomena such as the solar wind" (which incidentally has never been included in the analyses) would seriously inhibit the discovery of any new relationship.

A geomagnetic disturbance factor continues to be a systematic contribution to UFORs. It appears to enhance their numbers (if they are occurring) or contribute to their display (Persinger, 1983d, 1985). This condition is evident as the standard deviation of the monthly Ap (see the *Journal of Geophysical Research*) values for 6-mo. increments of analyses or the standard deviation of the daily Ap indices for monthly increments of analyses. At still

finer temporal windows, UFORS occur within a few days after enhancements of geomagnetic activity especially if they occur after several quiet days or weeks with minimal daily  $A_p$  values (the relationship between the magnitude of the geomagnetic disturbance and the latency and numbers of UFORS has not been evaluated). However, the geomagnetic factor during the same increment as UFORS is most pronounced if earthquake activity increases or occurs later within the area. This pattern has been interpreted as indicating an enhancement of the UFOR process by the geomagnetic factor if increases in strain are occurring in the area.

The geophysical basis for the hypothesis is *not* "extremely dependent upon recent reports of luminous effects produced by strain on rocks during fracture tests." The critical test of the hypothesis is the systematic spatial and temporal coupling between UFORS and earthquake measures within a region. Rock-fracture experiments may be important for determining the physics of the mechanisms by which the phenomena might be generated on a larger scale (Persinger, 1984a). To date the hypothesis only indicates that the individual event is a consequence of the very brief localization of normal geophysical forces that in turn evoke electromagnetic-like (Persinger, 1980) and perhaps gravity-related transients (Persinger, 1984a). The requirement for either phenomenon or their mechanisms to be reproduced first within a laboratory before they can be considered robust, would eliminate meteorology, geology, and astronomy as well as major portions of other disciplines.

The major criticism of the statistical analysis is that given a sufficiently large space and time, "countless other variables may be present." Although this concern is certainly warranted, Rutkowski does not mention that there are several analytical techniques and "if-then" procedures by which the intervention of confounding variables can be attenuated and the specific tests of the hypothesis can be accomplished. Within the large-scale analysis (6-mo. increments), the maximum (positive) bivariate correlation occurred between seismic activity and the numbers of UFORS during the previous 6-mo. increment (Persinger, 1980) when strain is inferred to be increasing. Other lags or leads (UFORS after increases in seismic activity) were not statistically significant. In addition, if increases in stress (and strain) generate UFORS, then correlations between *relative changes* in seismic activity from the previous increment and changes in the numbers of UFORS should be and are evident (Persinger, 1983d, 1983g; Persinger & Derr, 1984).

Rutkowski's concern about the large area and time increments involved with the major analyses is certainly understandable, especially for individuals who want to predict UFORS. However, the requirement of such large space-time frames within which to see a phenomenon is not unique to the hypothesis. To perceive the phenomenon called an air mass, hundreds of kilometers are re-

quired. To observe many geophysical periodicities, very large increments of time are required. If the increment is too small then no apparent change is evident; if the increment is too large then different phenomena may be overincluded within the same observation (Persinger, 1980b).

The failure to specify a particular point  $p$  with an effect  $q$  does not necessarily challenge an hypothesis, particularly if one is working with aggregates, fields or gestalts, depending upon the discipline. For example, a condition may exist subtly over a large area, in fact, several hundreds of kilometers. Within it, there may be increased incidences of medical ailments ( $ps$ ) due to diffuse but elevated distributions of particular minerals within the soil. Although the  $ps$  may be discrete events, the demand for specific points for  $q$  would not be appropriate. Rutkowski's logic would devastate some portions of modern epidemiology.

More directly, his approach would repudiate the most recent developments for the forecasting of earthquakes. One promising precursor, the anomalous change in the ratio of the P-wave velocity to the S-wave velocity, may occur days to months before earthquakes; the duration of the anomaly appears proportional to the amount of seismic energy that is ultimately released. However, these transient phenomena are reliable for only some types of quakes. Similarly, local alterations in radon gas in well waters may occur over large areas, weeks to months before the quake. Many of these changes may occur more than a hundred kilometers from the imminent epicenter (Hamada, 1981; Rikitake, 1976).

Any interest in relating single UFORs or clusters of UFORs within a small area to specific epicenters or aggregates of epicenters requires a *change* in the level of conceptual analyses. If UFORs within a small area, for example, 2500 km<sup>2</sup> are targeted, then the problem of distance for inclusion of epicenters should be critical. As Rutkowski aptly suggests, such tests (if the seismic data were inclusive and the sample of UFORs were representative) would clearly show the earthquake factor. Two recent analyses (Persinger & Derr, 1985)<sup>2</sup> for the Uinta Basin and for the Toppenish region (both about 2500 km<sup>2</sup>) indicate that UFORs clearly occurred during the same months as increased seismic activity within about 100 km from the central area of UFORs. Contemporary seismic activity at distances outside this area were not correlated significantly with the numbers of UFORs. Interestingly, the intervals between shifts in coordinates of successive epicenters that crossed the UFOR area were most frequently associated with UFORs.

Concern over selection of data for any analysis is certainly a critical consideration. However, dismissal of a data source, such as FATE (Persinger, 1981a, 1981b) because it *appears* to be less credible than that of a scientific

<sup>2</sup>J. S. Derr and M. A. Persinger, Relations among natural nocturnal lights, geomagnetic activity, and earthquakes in southern Washington. (Manuscript in preparation)

journal is a value judgment. The test of the usefulness of a data base should be determined by empirical criteria. Despite problems within a data set, it may still maintain sufficient sensitivity to indicate variability from key stimuli. For example, mining accidents may be affected by many political and social factors, but the over-all variations are still sufficient to discern contributions from the first-order meteorological factors.

Hendry's (1979) comment, as quoted by Rutkowski, is also subject to the criticisms of after-the-fact evaluations. The statement that "the UFOCAT file cannot be used as a source of data because of the inherent flaws in the design" requires empirical verification and not armchair dismissal. Even if 90% of the data in the UFOCAT file are misidentifications of ordinary phenomena (a conclusion based primarily upon the *later* attribution of cause to the most conspicuous satellite or planetary stimulus at the time of the sighting), the remaining reports may represent samples of the types of UFORs that would be coupled with imminent earthquake activity.

These cases would still constitute thousands of responses. They would still vary upon a baseline of reports that were due to a myriad of other sources, including collection artifacts. Empirically, this possibility has been indicated by the tendency for the Y-intercept of the equations for UFORs-earthquake measures to be greater than 0 even when the standard error of the estimate is considered. Of course, the protrusion of seismic-related UFORs above the baseline presumes that it is more or less stable over time. This interpretation would be most valid with the analyses that involved 6-mo. increments over several years if we assumed that (1) most nonseismic UFORs were randomly distributed within each increment and (2) the earthquake-related UFORs would be enhanced by the process of summing them within each increment.

The solution to the problem of the source of data is complex but tractable. There are a variety of internal checks that the proficient problem-solver can perform. One test is the correlation between databases. Although there are weak (0.30) correlations between the UFOCAT and FATE data for numbers of UFORs within the central USA, measures of "flap periods" (that involve many UFORs over a few weeks within a region) from the two data sets yield much stronger (0.65) correlations. Equations for UFORs and seismic measures since 1947 predict historical periods of luminosities during the nineteenth and early twentieth century (Persinger, 1983e). Similarly, UFOR-seismic equations for areas for which there were consistent databases for "luminosities" and quakes during the nineteenth century predict peak years of UFORs since 1947 (Persinger, 1983c, 1984b).

On a more positive side, Rutkowski's appreciation for the difference between a UFOR and a UFO event is important. The weakest portion of the UFOR-seismic association is with the *validity* of the UFOR. The majority of

UFORs are based upon human observation and reporting contingencies. Relative to the numbers of UFORs, there are *very few* instances of corroboration by independent instrumentation such as photographs or films. It is possible that, throughout history, the reports of unusual lights have been associated with environmental stimuli that directly influence human behavior. Whereas the reliability of UFORs could approach 1 whenever these stimuli were present, the validity of UFORs could range from 0 to 1. What the person says he saw may have very little in common with the actual event.

From this perspective, a substantial portion of UFORs, even those that describe luminous and metallic-looking phenomena, are generated by the strain field and the geomagnetic factor through as yet unknown mechanisms. These UFORs would *not* be evoked by the traumatic stimulation discussed elsewhere (Persinger, 1983b). The popular literature (Keel, 1970; Pugh & Holiday, 1981) abounds with references to the tendency for not only different types of UFORs but other odd or Fortean events to occur in temporal clusters. Correlational analyses support these observations (Persinger, 1982; Persinger & Lafreniere, 1977). Whether or not geophysical variables elicit a general propensity to report odd things or actually evoke the alleged behaviors is an intriguing problem for the evolving field of behavioral epidemiology (Persinger, 1983a).

Rutkowski's questions and intrinsic commentary raise one final and sensitive issue that must be addressed; it involves the objectivity of a scientist who approaches a problem that is a part of his belief (Persinger, 1979a). According to Little (1984), almost 40% of the ufologists have concluded that UFORs are due to extraterrestrial sources (presumably "intelligent"). Yet there is almost no empirical analysis of UFO reports in the traditional scientific literature and for the most part the extraterrestrial explanation is stated as an empty hypothesis. It is defined in a manner that is not testable. Instead the conclusions are based upon "I believe" statements, "the strangeness of the phenomena" or the egocentric contention that "if a phenomenon deviates from what is known or plausible to the observer, therefore it is alien."

The multivariate nature of the problem of UFORs dooms any single hypothesis that attempts to explain all cases. There are many different sources of UFORs, even for those within the same descriptive category. Subsets of cases that most likely constitute fraud, psychopathology, or misidentification do not refute the hypothesis any more than subsets of healthy people who smoke for decades challenge the correlation between smoking and lung cancer. Unfortunately, the demonstration of the hypothesis within these data will remain a controversial issue that will be marred by the connotations of "statistical analyses" and by the intrinsic complexity of the methodology.

Definitive tests in the tradition of the physical sciences require more di-

rect measures of strain conditions and more systematic (preferably instrument-dependent) measures of UFORS. There have been very few areas for which both systematic data for UFORS and inclusive seismic information have been available. Quantitative analyses have just begun. As the density of seismic surveillance increases, the conditions for testing the hypothesis will become more optimal. Rutkowski's theme that the "theory is just too good to be true" may reflect healthy skepticism; however, this approach does not replace the requirement for refutations based upon empirical analyses rather than philosophical possibilities.

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